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Technical Note   124

PAPERS ON THE SYMPOSIUM ON COLLISION PHENOMENA IN ASTROPHYSICS, GEOPHYSICS, AND MASERS
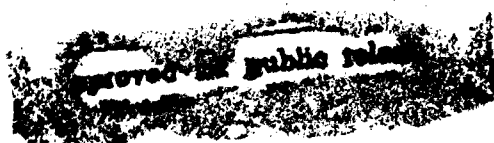
U. S. DEPARTMENT OF COMMERCE
NATIONAL BUREAU OF STANDARDS

THE NATIONAL BUREAU OF STANDARDS

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A complete listing of the Bureau's publications can be found in National Bureau of Standards Circular 460, Publications of the National Bureau of Standards, 1901 to June 1947 ($1.25), and the Supplement to National Bureau of Standards Circular 460, July 1947 to June 1957 ($1.50), and Miscellaneous Publication 240, Jı ' · 1957 to June 1960 (Includes Titles of Papers Published in Outside Journals 1950 to 1959) ($2.25); available from the Superintendent of Documents, Government Printing Office, Washington 25, D. C.

NATIONAL BUREAU OF STANDARDS

Technical Note

124

DECEMBER 1961

PAPERS FROM THE SYMPOSIUM ON
COLLISION PHENOMENA IN ASTROPHYSICS,
GEOPHYSICS, AND MASERS

M. J. Seaton, A. Dalgarno and C. Pecker

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TABLE OF CONTENTS

Papers from the Symposium on Collision Phenomena in

Astrophysics, Geophysics, and Masers

This publication comprises three papers on
astrophysical and geophysical problems that were
presented at a special symposium at the National
Bureau of Standards Boulder (Colo.) Laboratories
in June 1961. The speakers spoke on the question:
What are the most important atomic and molecular
data needed by theoreticians for progress in astro-
physics, geophysics, and gas lasers? The papers
are entitled "Astrophysical problems" by Michael
Seaton; "Collision processes in the high atmosphere"
by A. Dalgarno; and "Some problems connected with
the analysis of the structure of the solar atmosphere"
by Charlotte Pecker.

INTRODUCTION

The second International Conference on Electronic and Atomic
Collision Phenomena was held at the University of Colorado, in Boulder,
June 16, 1961. Because of the great interest of the National Bureau of
Standards not only in atomic physics but also in the basic physics of
astrophysical and geophysical phenomena, the NBS Atomic Physics Division
took the opportunity to prevail on four distinguished scientists,
present at the conference, to speak at a special symposium at the NBS
Boulder Laboratories to which all conference participants were invited.
The speakers were Dr. Michael J. Seaton, University College, London;
Dr. A. Javan, Bell Telephone Laboratories; Dr. A. Dalgarno, Department
of Applied Mathematics, Queens University, Belfast; and Professor
Charlotte Pecker, Institut d'Astrophysique. The speakers were asked to
speak to the question: What are the most important atomic and molecular
data, such as oscillator strengths, cross sections, and reaction rates,
needed by theoreticians for progress in the fields of astrophysics
(particularly stellar atmospheres), geophysics (aeronomy and ionospheric
physics), and gas lasers?

In the interest of stimulating a lively discussion with the audience (which occurred but is not recorded here), the speakers were asked to speak informally, without manuscripts. We are grateful to the High Altitude Observatory of the University of Colorado for arranging that the talks would be tape recorded. Speaking for the atomic physicists present, we found the discussion so stimulating that we prevailed on those speakers who dealt with astrophysical and geophysical problems to let us reproduce their remarks for the benefit of our colleagues in the NBS Laboratories for Astrophysical and Plasma Research who could not be present. Hence, these talks should be treated as unpublished material, and the speakers must not be held responsible for this version of their remarks which has survived transcription from tape and blackboard.

I feel that these talks provide an excellent guide line for the NBS program of research in "laboratory astrophysics", or as Minnaert called it, the "physics of astrophysics". I hope that other atomic physicists will also feel challenged by the gauntlet which has been thrown down to us.

I am pleased to express my gratitude not only to the speakers, but to Dr. S. J. Smith and Dr. R. N. Thomas who did most of the work in the preparation of this written version of the proceeding. We are also indebted to Dr. F. W. Brown, Director of the NBS Boulder Laboratories, for his warm welcome to the conference delegates and for making available to us the facilities of the NBS Boulder Laboratories.

Lewis M. Branscomb
Chief, Atomic Physics Division
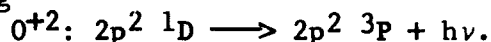Washington, D. C.

1. ASTROPHYSICAL PROBLEMS

Dr. Michael Seaton

University College, London

I am going to talk about some astrophysical problems in which atomic collision processes are important. First, I shall discuss one of the simplest astrophysical problems, that of the gaseous nebula. Consider a gas cloud near a hot star and, the density of the gas being very low, only fairly elementary processes are of importance. The ultraviolet radiation of the star ionizes the gas. To give an idea of the orders of magnitude: the distance of the gaseous nebula from the star is 10^{17} cm, or 10^6 times the radius of the star; the electron density is $\sim 10^4$ cm^{-3}; and the kinetic temperature of the gas is $\sim 10^4$ °K. The physical problem is to determine how the gas behaves and, in particular, the spectrum which the gas emits when excited by the stellar ultraviolet. For the astrophysicist, the problem is posed the other way round. He observes and measures the spectrum and then deduces the density, temperature, and chemical composition of the gas and also the nature of the ultraviolet radiation field.

It turns out that hydrogen is the most abundant element in the nebula. It is ionized by the ultraviolet, and one observes the spectrum emitted on recombination. To calculate the intensities in a recombination spectrum, one must obtain the rate coefficients for capture on all the excited states and then determine how the atoms cascade down to lower states. The problem can be solved exactly for hydrogen, and for complex atoms quite good approximate solutions can be obtained.

One observes, in addition to the hydrogen lines, forbidden lines, the strongest in most nebulae being
$$O^{+2}:\ 2p^2\ {}^1D \longrightarrow 2p^2\ {}^3P + h\nu.$$
The excitation potential is 2.5 ev. The line is excited by electron impact; at 10^4 °K the mean electron energy is $kT \sim 1$ ev and an appreciable number of electrons have energies of 2.5 ev or more. The upper level is metastable, and the transition probability for the line is 0.03 sec^{-1}, compared with something like 10^8 sec^{-1} for a strong permitted line. But as long as the density is low enough the metastability does not matter; each excitation is still followed by quantum emission.

I am going to discuss this in a little more detail because it provides an introduction to the idea of local thermodynamic equilibrium (LTE). Consider an atom with two levels, a ground level and an excited level. Let N_1, N_2 be the number of atoms per cm^3 in these levels and let N_e be the number of electrons. The processes to be considered are collisional excitation and spontaneous emission of radiation. One can neglect absorption and induced emission of radiation because the radiation density is low and the nebula is optically thin in the line.

-3-

Let the radiative transition probability be A_{21} and the collisional transition probabilities be $q_{12} N_e$, $q_{21} N_e$, where

$$q_{ij} = \overline{(v_i Q_{ij})},$$

v_i is the initial electron velocity, Q_{ij} is the cross section, and the average being over the Maxwellian distribution. From detailed balancing one has

$$q_{12} = \frac{\omega_2}{\omega_1} q_{21} e^{-E_{21}/kT}$$

where $E_{21} = (E_2 - E_1)$ is the excitation energy and the ω's are statistical weights.

The equilibrium equation is

$$N_1 [q_{12} N_e] = N_2 [A_{21} + q_{21} N_e]$$

and the quantum emission ($cm^{-3} sec^{-1}$) is

$$S_{21} = N_2 A_{21} = N_1 \left\{ \frac{q_{21} N_e}{A_{21} + q_{21} N_e} \right\} A_{21}.$$

Now let us define a critical density N_e^c such that

$$q_{21} N_e^c = A_{21}.$$

We have

$$S_{21} = \begin{cases} N_1 q_{12} N_e & \text{for } N_e \ll N_e^c \\[2ex] N_1 \left\{ \frac{q_{12}}{q_{21}} \right\} A_{21} = N_1 \frac{\omega_2}{\omega_1} e^{-E_{21}/kT} A_{21} & \text{for } N_e \gg N_e^c. \end{cases}$$

In the low density case, each excitation is followed by quantum emission and in order to interpret the spectrum one must know the collision cross section. The radiative transition probability does not enter the expression for the line intensity.

In the other extreme, the high density limit, most collisional excitations are followed by collisional deactivations. In calculating level populations, radiative transitions can be neglected and the relation between q_{12} and q_{21} gives us the Boltzmann equation,

$$\frac{N_2}{N_1} = \frac{\omega_2}{\omega_1} e^{-E_{21}/kT}$$

A gas is said to be in LTE when a temperature T can be defined at each point in the gas and when the gas satisfies the thermodynamic equilibrium equations:

(i)   The Maxwell equation for velocities
(ii) The Boltzmann equation for level populations
(iii) The Saha equation for the ionization equilibrium.

In gaseous nebulae, the Maxwell equation is satisfied in a very good approximation, because of the effectiveness of collisions in redistributing kinetic energies. The Boltzmann equation is approached for some of the most highly metastable levels. But the Saha equation never applies at all. Collisions never contribute appreciably to ionization processes, since the kinetic energies are very small compared with the ionization potentials. Photoionization is much more important, and, since the radiation density is nothing like that for thermodynamic equilibrium (the Planck density), the ionization equilibrium is quite different from that in LTE.

In astrophysics one frequently has the position that the radiation density is not given by Planck's law, essentially because one has an object of a finite size and the radiation can, more or less freely, escape into space. Nevertheless, conditions of the material particles may approach those for LTE provided that the density is high enough for collision processes to be more important than radiative processes. In order to know the value of the critical density above which LTE may be assumed, it is necessary to know the collision cross sections. But once one is satisfied that LTE can be assumed, then collision cross sections are no longer needed. To interpret the spectrum one needs only the LTE equations and the radiative transition probabilities.

Returning to the problem of forbidden lines in nebulae, we find that N_e is much smaller than N_e^c for the strong O^{+2} line, but that $N_e \sim N_e^c$ for a number of other forbidden lines. To interpret the spectrum we need both cross sections and transition probabilities. In its way this is a fortunate situation. The line intensities depend on both T_e and N_e and, from the observed intensities, both T_e and N_e can be deduced. Fairly good atomic data are available for ions, such as O^{+2}, with outer $2p$ electrons; but improvements are still needed. Much more work should be done concerning ions with outer $3p$ electrons, such as S^{+2}.

In the first talk in the conference which has just ended, Roy Garstang spoke about his work on transition probabilities in Fe^+. Forbidden iron lines occur in nebulae and also in the spectra of certain peculiar stars. Garstang has made a great deal of progress with the transition probabilities, but as yet no one has attempted to obtain cross sections for these cases. This work will not be easy, but eventually it should be done.

Leaving the problems of nebulae, let us now consider some solar problems. We observe the sun but have little knowledge of the physical conditions which prevail in the sun's atmosphere. The only way to progress is to start with some very bold assumptions. Then one gets a feel for the problem, a first idea about what the physical conditions really are. One may then compare theory and observation, go back to question the assumptions made, and continue to make improvements and refinements until finally one can build up a fairly well-detailed picture of what the sun is like.

The usual first approximation assumes
(i)   LTE
(ii)   No conduction and no convection; hydrostatic equilibrium
(iii) An absorption coefficient, κ, independent of frequency.
Already the model gives quite tolerable agreement with a number of observed properties of the solar spectrum. One feels that it is a good start. The optical depth τ is defined by $d\tau = -\kappa dr$ where r is the distance out from the center of the sun. Our first model gives the local temperature T as a function of τ (it goes roughly as $T^4 = (1 + 3\tau/2) \, T_0^4$, where T_0 is the surface temperature). But we cannot get the actual depth, or the gas pressure, as a function of τ because we have not as yet related the absorption to the physical properties of the gas.

In a second approximation we drop the assumption that κ is independent of ν. From the observed spectrum the ν-dependence of κ may be deduced. At first the result obtained was very puzzling but, following a suggestion by Wildt, it was eventually established that the absorption is mainly due to the hydrogen negative ion in both bound-free transitions

$$H^- + h\nu \longrightarrow H + e$$

and free-free transitions

$$H + e + h\nu \longrightarrow H + e.$$
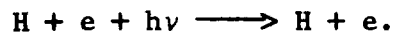
To calculate κ_ν, one must study electron hydrogen collisions, a subject which has been discussed at length. Lewis Branscomb showed some of the latest results, both theoretical and experimental, for the bound-free absorption coefficient. Some very accurate calculations have been made, and I think that the physicists can say to the astrophysicists that, within a year or two, the H^- absorption cross sections will be known to an accuracy of several significant figures. Good accuracy is well worth attaining. In constructing models of the solar atmosphere one must consider that movements may be important, that there may be departures from LTE, and so on. It is a big help if one does not also have to be concerned about the possibility of errors in the basic atomic data.

Having identified the absorption process, we may calculate κ_ν when we know the abundance of H⁻. In LTE this is given by the Saha equation

$$N\,(H^-) = \frac{N_e N_+}{4} \left[\frac{h^2}{2\pi m k T} \right]^{3/2} e^{I/kT}$$

where I (= 0.75 ev) is the detachment energy. Again using the Saha equation, one finds that there is very little ionized hydrogen, but that the elements with small ionization potentials (the metals) are ionized. This provides the free electrons to form H⁻. When the metal abundance has been determined from the interpretation of the line intensities, one obtains $N_e \sim 10^{-4} N\,(H)$. One is now in the position to get κ_ν as a function of pressure and temperature and hence to get a complete model of the solar atmosphere (temperature and pressure as a function of depth in cm).

The question arises as to whether it is correct to use the Saha equation for the metals and for H⁻. For the metals, the complication arises that there are some uncertainties in the procedures used to obtain convergence in the sums for partition functions. More work needs to be done on this. The question of LTE for H⁻ has been discussed by Bernard Pagel (Mon. Not. R. Astr. Soc. 119, 609, 1959). Calculations by Syd Geltman show that

$$H^- + e \rightleftarrows H + e + e$$

is not important in the higher layers. Pagel considers

$$H^- + H \rightleftarrows H_2 + e.$$

A cross section estimated by Alec Dalgarno shows that this process is an important one. It will give LTE for H⁻ if one has LTE for H_2, but the question as to whether LTE holds for H_2 can be answered only by considering all the other processes leading to formation and destruction of molecules.

Similar problems, as to whether or not LTE can be assumed, arise in interpreting the Fraunhofer absorption lines in the solar spectrum. These will be discussed by Charlotte Pecker.

Let us now consider the outermost parts of the solar atmosphere. At the limb of the sun (the apparent edge of the disk observed in the light of the whole sun), the temperature reaches a minimum value of $T_0 \sim 4400°K$ and then begins to rise again. This region is the chromosphere. Still farther out one comes to the corona, where the temperature is 10^6 °K.

The problem of the chromosphere is very difficult. Conditions are such that LTE cannot be assumed, and yet the density is not so low that, as in the case of nebulae, one can fairly easily pick out a small number of processes which really determine the situation. I will not discuss this further except to mention one type of collision process that may not be mentioned otherwise. The chromosphere is heated by a process involving dissipation of energy in shock waves and treating this problem requires ionic mobilities. Proton-hydrogen charge exchange,

$$H + H^+ \longrightarrow H^+ + H$$
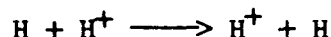
is of importance.

Problems in the corona are nearer to the types of problems which occur for nebulae, but the density and temperature are both higher (10^{10} cm^{-3} and 10^6 °K). The coronal spectrum contains lines of Fe^{+9}, Fe^{+13}, and so on. The ground configuration of Fe^{+13} is $1s^2 \, 2s^2 \, 2p^6 \, 3s^2 \, 3p \, ^2P_J$. The fine structure is so large that the transition $^2P_{3/2} - ^2P_{1/2}$ gives a line in the visible spectrum, in fact, one of the strongest lines from the corona. Electron impact excitation is important in determining the line intensities. Further work is needed on calculations for this type of collisional transition.

Considering the ionizing equilibrium in the corona one finds that only two processes are of real importance: collisional ionization,

$$X^{+m} + e \to X^{+(m+1)} + e + e,$$
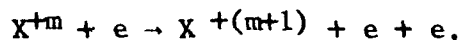
and radiative recombination,

$$X^{+(m+1)} + e \to X^{+m} + h\nu.$$
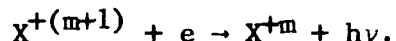
The ratio $N(X^{+m}) / N(X^{+(m+1)})$ therefore depends only on T and not on N_e. Comparison of calculated and observed ionization equilibria gives a temperature of a little less than 1×10^6 °K. On the other hand, the line profiles, interpreted assuming thermal Doppler broadening, give 2×10^6 °K or more. In some astrophysical problems a discrepancy by a factor of 2 or 3 would not be too serious, but here it means that something is definitely wrong. If it were due to errors in the cross section, then the cross sections would be in error by factors of 30. The work described at the conference, by Alan Burgess and by Eleonore Trefftz, makes it seem most unlikely that the error in the cross sections could be nearly as large as this.

The discrepancy in the temperature obtained by different methods I will leave as one of the problems which has not yet been cleared up to everyone's satisfaction.

In conclusion, I should like to say something about atomic data needed in astrophysics. Every sort of approach is needed. In some respects, theory is more versatile than experiment. The theorist does not avoid beryllium because it is poisonous and he is not worried by the fact that the manufacturers do not supply cylinders of atomic oxygen. On the other hand, there are problems such as those of heavy atoms, of atom-atom collisions, and of molecules - where the theoretician has little hope of getting precise numbers. It is important to have an all-around attack on many of these problems. Sometimes the theory can provide approximate formulae for estimating cross sections, and it is then important to have a few good experimental results for selected cases which enable one to check the real worth of the theory.

2. COLLISION PROCESSES IN THE HIGH ATMOSPHERE

Professor A. Dalgarno

Department of Applied Mathematics

Queens University, Belfast

The subject of my talk is "Collision processes in the high atmosphere". By atmosphere, I mean the earth's atmosphere; however, the general problem presented is similar for any of the planets. The problem can be posed very simply: Consider an atmosphere (in the case of the earth, composed largely of nitrogen and oxygen) moving in gravitational and magnetic fields and bathed in solar radiation. The first problem is to explain the chemical constitution, and the second is to explain the luminosity that is observed to arise in the atmosphere. A third problem concerns the ionosphere - to explain how it is produced, how it is maintained, and what its effects might be.

Largely as a result of the work of Chapman, Massey, Bates and Nicolet, we now have a broad understanding of the main features of the earth's atmosphere. Detailed understanding requires, in addition to many more careful observations of atmospheric and solar phenomena, accurate values of the rates of those collision processes which theoretically may be significant. There are a very large number of possible processes which may or may not be significant. One must look at those which may not be, as well as those which may, in case one has made an incorrect choice. Although in recent years considerable advances have been made in providing reliable values, much remains to be done. I shall try to present the major uncertainties. I shall not say very much about why particular processes are important, but will simply stress those that are.

The action of solar radiation is essentially to cause dissociation and ionization of the atmospheric gases. Quite apart from the intensity of the solar radiation, we need to know the atomic parameters - the photodissociation cross sections and the photoionization cross sections for all wavelengths of concern. The photoionization and photodissociation of the molecular gases O_2 and N_2 have been measured, mainly by Weissler and his colleagues, for wavelengths from the spectral heads at around 1000 Angstroms down to wavelengths of the order of, say, 1 Angstrom. Weissler uses a line spectrum source, so that, in fact, what one has are the cross sections at various selected wavelengths. There must be some very considerable structure in these curves which is hidden by this method of determining them. What one really needs, in addition, are measurements of these ultraviolet absorption rates using a continuous source. I do not really know what is practicably possible, but electron Bremsstrahlung does provide a nice continuous source. I might also say one would like to see Weissler's work repeated. I am not suggesting that it isn't accurate, but rather that one always feels better if two people from different places arrive at the same result. (Actually, there are discrepancies with some French measurements.)

In the case of atomic oxygen and atomic nitrogen, there are no measurements - no reliable measurements - but there are some quantal calculations. (Roughly speaking, we may say that if it is an atom we can hope at least to do something about the calculations; if it is a molecule, by and large we cannot really hope to do anything.) In the case of the atoms, atomic oxygen and atomic nitrogen, there are quantal calculations by Bates and Seaton. These are sufficiently accurate for our purposes in the region near the spectral head at, let us say, 900 Angstroms. We also have accurate values in the short X-ray region - less than 20 Angstroms - where the problem reduces to a rather simple one. What we do not have are reliable values between these limits. There are some essentially empirical values due to Nicolet, and now there are some rough calculations by myself and Parkinson - perhaps slightly better, but not very much. What is needed is the extension of the Bates-Seaton calculations to these wavelengths. This is something one can do theoretically (and unless somebody else does it, I shall have to). This region I am speaking about is the region in which the inner shell electrons of the atom are ejected - the inner 2s shell. It is rather important that it be handled correctly and precisely, since the region contains two of the very important solar lines - one at 304 Angstroms and the other at 584 Angstroms; the first arising from He^+ and the second, from helium.

Let me turn now to consider the dissociation products as the solar radiation is turned off and recombination processes become predominant. We consider first the chemical processes; these are the main reason the upper atmosphere is, in a sense, much more complicated in its details than some of the astrophysical problems we have been hearing about, in that we are entering chemistry. We have fairly good values for those reactions which occur in a pure oxygen atmosphere. In three-body recombination,

$$O + O + M \longrightarrow O_2 + M, \qquad (1)$$
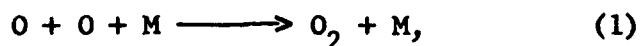
where M is any third body, essentially nitrogen. Also,

$$O + O_2 + M \longrightarrow O_3 + M. \qquad (2)$$
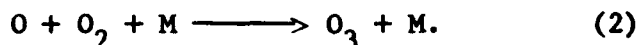
An auxiliary reaction enters if ozone is present:

$$O + O_3 \longrightarrow O_2 + O_2. \qquad (3)$$
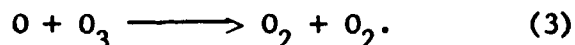
These are the three essential reactions and quite reliable values are available for them. We would like a little more information on how reaction (1) varies with temperature. The measurement was made at $300^\circ K$.

We have values for the rates of these processes, but actually these are not enough. What we really need to know for a detailed understanding and explanation of much of the airglow phenomenon is in what states - excited electronic and vibrational states - the end products are. There is no information on this at all, and I do not see that we will be able to provide theoretically anything but a broad indication. Some experimental work must be done. It appears to be very difficult - but somehow or another the state of the end products has to be examined. Certainly it does not seem too difficult to examine the vibrational population; you can do this by an absorption experiment. When one recognizes that there is some nitrogen to be considered as well, and that atomic nitrogen tends to form, you find a lot more reactions entering. (The molecular nitrogen does not really cause any problem, only the atomic form.)

Things of this kind occur:

$$N + O + M \longrightarrow NO + M. \tag{4}$$

The oxygen and nitrogen tend to combine in an important reaction which produces nitric oxide. NO is a minor but very important constituent of the atmosphere. We can eliminate the nitric oxide by the reaction with atomic nitrogen:
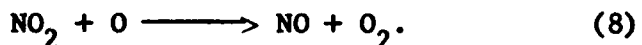
$$N + NO \longrightarrow N_2 + O \tag{5}$$

to get N_2 again. The important process, though, is:

$$N + O_2 \longrightarrow NO + O. \tag{6}$$

This is a fairly fast reaction. We can convert the NO in the three-body recombination:

$$NO + O + M \longrightarrow NO_2 + M, \tag{7}$$

and can also change the NO_2 which is formed, by a reaction with atomic oxygen:

$$NO_2 + O \longrightarrow NO + O_2. \tag{8}$$

We now know the ratios of most of these reactions at 300°K. What we want actually is to know more precisely the variation with temperature. We also want, as I remarked before, the electronic and vibrational states of the end products. Those of you who are interested in the details of this set of reactions - which are the most important ones in an atmosphere of nitrogen and oxygen - will find them reviewed and listed in a recent paper by Barth. There is also in the atmosphere, as you may have observed, some water vapor which is photodissociated,

$$H_2O + h\nu \longrightarrow H + OH \tag{9}$$
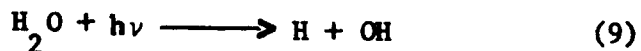
to provide the source of atomic hydrogen which becomes an important minor constituent of the atmosphere lower down, and a major constituent highe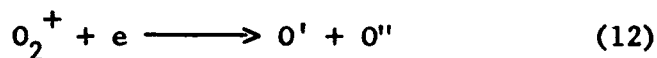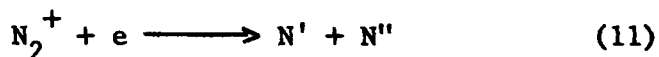r up. When hydrogen is introduced into the chemistry of the atmosphere, the structure becomes extremely complicated. To list the possible reactions would be extremely time-consuming, but I think it is acceptable to say that we know rather little about the rates of the various possible chemical reactions when atomic hydrogen is present. Of particular interest is the reaction

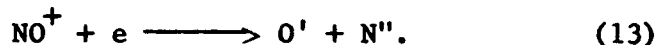$$O + H \longrightarrow O_2 + OH \qquad (10)$$
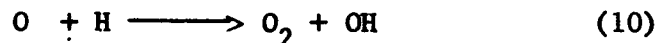
since this is at least partly, though it may not be entirely, the source responsible for the most intense features of the airglow spectrum - the rotation-vibration bands of the OH radical. I would like to know the rate of this reaction, for once one can get accurate values of the rate of those reactions which lead to features of the airglow that are observed at night, observations of airglow become a very powerful means of determining and understanding quantitively the detailed processes of the atmosphere. (Airglow observations are a fairly routine business now; they are available in large number from all parts of the world.) But until one knows the rates of the processes which lead to the airglow, this cannot be done. This is really all I am going to say about chemistry.

I go now to the central subject of the recently concluded collision conference: What happens to the ions and electrons which are produced by photoionization? The most important process that we don't actually know very much about yet is dissociative recombination, despite the very large amount of experimental attention it has received. It was suggested about 10 years ago by Bates that this might be a very fast recombination process and this, at least, has been established. The processes we are concerned with are, of course,

$$N_2^+ + e \longrightarrow N' + N'' \qquad (11)$$

$$O_2^+ + e \longrightarrow O' + O'' \qquad (12)$$

and
$$NO^+ + e \longrightarrow O' + N''. \qquad (13)$$

There have been many measurements of the rate of reaction (11) - or, rather, let me say there have been many measurements of the rate of an apparent recombination process which occurs with nitrogen gas in the laboratory. It is my belief that none of these measurements applies to N_2^+. What is needed, of course, is some means of identifying this ion. (I think the answers may be right all the same, but they probably do not refer to the ion N_2^+.)

In their most recent measurements, Champion and Faire determine apparent recombination coefficients at the same time that they are measuring diffusion coefficients - an ambipolar diffusion coefficient. If one takes the ambipolar diffusion coefficient and extracts or obtains from it the mobility of the ion, which one can do very simply, one finds that the ion mobility is just that observed by Tyndall some 20 years ago. If one looks a little further into what Varney and Beaty and others have been doing on the mobility of ions in nitrogen, there seems little question that the ion that Tyndall was observing was N_4^+, not N_2^+. This leads inescapably to the conclusion that wheatever Champion and Faire were measuring, they have an awful lot of N_4^+. It would, incidentally, be very interesting to know what the dissociative recombination coefficient is for N_4^+. There is the interesting possibility that this ion can be formed in the atmosphere, and that it may survive for considerable time. Information concerning reactions of this kind that lead to these quite complex ions would help one to decide whether or not they are present in the atmosphere.

One of the papers this afternoon, in the unclassified section of the classified conference, concerns NO^+ + e, dissociative recombination. There have been measurements by Sayers of this reaction at relatively high temperatures - 2000°. These are probably acceptable in the sense that at such temperatures, complex ions such as N_4^+ do not survive, but are dissociated. I would remark that in measuring such a process, which for some reason is extremely difficult, we do want to know also the temperature variation. It is probably rather rapid; it is suggested that a variation of the sort $T^{-3/2}$ or $T^{-1/2}$ is possible. One needs to know this variation because in the atmosphere we do have a range of temperatures - shall we say, 200 to 2000 or 3000°K. A decreasing number of people say we need to know the variation for higher temperatures; I think 3000°K will suffice as an upper limit. The variation is important because there are suggestions that in the polar regions and during aurora, or perhaps all the time, the temperature is very much higher than in moderate latitudes. And if the temperature is very much higher, then there are going to be effects that could conceivably be explained by simple variation with temperature of recombination rates. Perhaps some of the apparent anomalies can be attributed to this type of effect. The recombination rate may go down quite sharply - by a factor of 100, perhaps. This is surely going to be very important. However, the thing one would really like to know (and here I suppose it is not technically feasible) is what states are excited after this process. Unfortunately, the energetics are such that the levels populated are metastable; this is very difficult to determine, but it is very important. I am not sure who made the suggestion - it may have been Mike Seaton - that the famous red line in the airglow spectrum is a consequence of reaction (12). Nicolet had suggested that the green line was, but in fact, the red line is (and partly of (13) as well). So one would like to know whether one can get the red line from these processes. One can energetically, of course, and, further, one would like to know the specific rate for the production of one quantum of red radiation.

-14-

Leaving the recombination for a moment, let us consider something rather simpler for which we still need more measurements. Radio waves are absorbed and scattered by electrons and to a lesser extent by ions in the atmosphere, and the amount of absorption is essentially controlled by the elastic scattering of the electrons by the atmospheric gases. We do have rather reliable values for elastic scattering cross sections of electrons by nitrogen molecules. And at this conference I learned that we also have a value - probably reliable - of the elastic scattering of electrons by atomic oxygen down to about 2 electron volts. We actually want them at lower energies than this; we are essentially concerned with room temperature. What we do not know is the contribution of the molecular oxygen, as this has never been measured down to the energies with which we are concerned - a fraction of an electron volt. Measurement of this contribution is very difficult because of the attachment processes, but it is certainly an important quantity. Given these cross sections, we can predict very closely the amount of absorption expected from a radio wave of a given frequency.

Another quite closely related phenomenon is the rate of cooling in the atmosphere of electrons which have been warmed up in some way, such as by an ordinary radio wave. The electrons are cooled predominantly, at least the low energy electrons are, by vibrational and rotational excitation of molecular nitrogen and molecular oxygen. Schulz has provided us with some very valuable data on what is essentially the rate of cooling by vibrational excitation in molecular nitrogen. Phelps and others at Westinghouse have now given us values of the cross section for the rotational excitation of nitrogen. We need to know the values for oxygen as well. A discrepancy exists between theory and experiment as far as the rotational excitation is concerned, and this is something that we will have to study theoretically much more carefully.

The initial step in the sequence of processes which is now regarded as probably being responsible for recombination in the F-layer high in the atmosphere is ion-atom interchange, as suggested by Bates from analogy with chemical reactions. The processes are

$$O^+ + N_2 \longrightarrow NO^+ + N \tag{14}$$

$$O^+ + O_2 \longrightarrow O_2^+ + O. \tag{15}$$
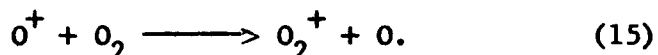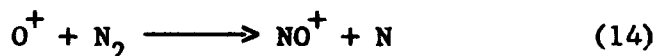
The second is similar, as far as the end products are concerned, to charge transfer, but it is not charge transfer. To underline the difference, charge transfer would be

$$O^+ + O_2 \longrightarrow O + O_2^+. \tag{16}$$
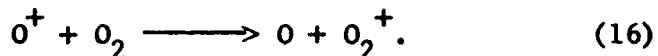
These two, (15) and (16), are not the same, though, in effect, of course, they are. The suggestion is that the reactions are essentially (14) and (15) which give NO^+ or O_2^+ and then dissociative recombination completes the recombination processes. Notice how similar they are to reaction (6), and it is just this similarity which made Bates suggest this process might be quite rapid. When theoretically one suggests that a process might be quite rapid, experimenters look at their data again and they find, in fact, that they knew this all the time. This happened with reaction (14), and a rate of 10^{-8} cm^3/sec was suggested. Actually, this is impossibly fast. One has to be very careful in interpreting measurements where so many reactions are all going on at the same time. Process (15) has been measured by Dickinson and Sayers in Birmingham. They obtained a rate something like 2.5×10^{-11} cm^3/sec, which is certainly a good deal further in the right direction. But according to Bates and Nicolet, the reaction cannot be this fast; essentially because if it were, the F-layer would not survive the night, and it does. Hertzberg at Lockheed has argued that Bates and Nicolet are in error. It would be helpful if somebody else would measure reaction (15) - and of course also (14), which is the more important. There are several others of interest which I will not bother to list - all are listed in a paper by Bates and Nicolet, that is, all those that are exothermic and can proceed under atmospheric conditions. We need to know them all, though I think (14) and (15) are the two which really matter.

In the D-region of the ionosphere, getting farther down to, say, 70 km, the situation becomes interesting because it seems that in that region the electrons are mainly removed by attachment processes and not by recombination processes. The attachment is to oxygen, because nitrogen in the ground state has no negative ions. There are, of course, both O^- and O_2^-. As far as O^- is concerned - largely as a result of the work of Branscomb and his colleagues at the NBS - we seem to know all we need to know. The photodetachment cross section of O^- has been measured, and from that one can find the coefficient of radiative attachment. When we come to reactions involving O_2^- and other particles, the situation is much less clear. Some of the confusion, at least, has been removed in the last two or three years by the work of Biondi, Phelps, Branscomb and colleagues, and Van Lint of General Atomic. The most important attachment process seems to be three-body attachment according to the process

$$O_2 + O_2 + e \longrightarrow O_2^- + O_2. \qquad (17)$$
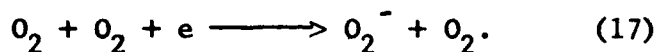
This seems to have a large rate at D-region temperatures ($240^\circ K$): 3×10^{-30} cm^6 sec^{-1}. This result helps a great deal in building up a picture of what really happens in the D-region. It is insufficient, however, to explain all the data that are available; the meteor data (at about 95 km), in particular, suggest that there is another relatively efficient attachment process. The observations cannot be explained by (17), simply because the molecular oxygen density decreases too rapidly there. It may be that the interpretation in terms of attachment is incorrect.

A very interesting problem is presented by some work of Bailey - largely with Branscomb - on what happens during polar blackouts. Polar blackouts are caused by solar cosmic rays. In what I regard as a remarkable piece of work, starting from a most complex situation, Bailey and Branscomb were able to deduce a reaction rate - a very complicated way of performing an experiment. This reaction rate was for a detachment process. The detachment process is not specified, but some sort of detachment is going on. The natural suggestion is that it is

$$O_2^- + O_2 \longrightarrow O_2 + O_2 + e \qquad (18)$$
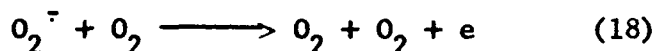
or something of that kind. The value they got is 2×10^{-17} cm^3/sec; which is pretty small, but not really small enough. Phelps measured this reaction and got a rate several orders of magnitude smaller for the same temperature. What, then, is the detachment process that is going on?

I have been wondering since hearing of Phelps' measurement whether, indeed, there is a lot of atomic oxygen around. Beginning with the impact of the solar cosmic rays, there are a large number of ions (N_2^+, O_2^+) formed. They dissociate by recombination and yield plenty of oxygen and nitrogen atoms. And I think - Bailey, I know, agrees - that we have to look rather carefully at the possible contribution of these atoms. The point is that if sufficient O is available, there is a possibility of this reaction:

$$O + O_2^- \longrightarrow O_3^- + e. \qquad (19)$$
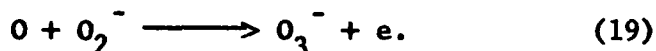
This may be quite an efficient mechanism; we certainly would like someone to measure it. I think it would be quite fast. We would also like to know what the radiative attachment to O_2 is, of course. Dr. Donahue remarked that the apparent contradiction between Branscomb - Bailey, if you like - and Phelps might be due to the fact that the O_2^- or the O_2 is not the same in the experiment and in the atmosphere, there being a possibility of vibrational excitation. This is one of the things we would certainly like to know. There are a number of suggestions now current that vibrationally excited O_2 may be an important constituent in reactions leading to the airglow as well. So one would like to know the lifetime of the vibrationally excited O_2 molecule and how many collisions it can suffer before it is deactivated.

I have been talking about the basic processes of an atmosphere that is bathed in solar electromagnetic radiation. Of course, there are a large number of other possible radiations, and one I have not mentioned is particle radiation - essentially electrons and protons - which are responsible for the aurora. I will not try to consider the aurora. There one is moving into an energy range quite different from that which I have been discussing, that of thermal processes. An aurora is due to high energy processes. One needs measurements of inelastic cross sections by electron and proton impact on the gases of molecular oxygen and molecular nitrogen and possibly atomic oxygen. It would be rather interesting if it could be done also by alpha-particle impact. Much of the information on molecular nitrogen has become available through the work of D. T. Stewart and, more recently, of N. P. Carleton. This work is continuing. It is important that we should have data not just for molecular nitrogen but also for molecular oxygen so that we may hope to come to some rational explanation of the auroral phenomenon.

That is really all except for one or two additional remarks which I am not really going to explain. One would like to have more measurements on a process which does matter in the atmosphere:

$$O^+ + H \longrightarrow O + H^+. \tag{20}$$
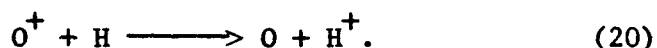
This happens to be a virtually exact resonance process - accidental resonance. There are two theories about what the magnitude of this cross section should be at thermal energies, where it is important. One theory says that in the limit of zero energy, the cross section is zero; the other says that in the limit of zero energy, the cross section is infinite. The answer probably lies between the two. It is difficult to do this sort of thing experimentally, and surely we must reconsider this theoretically.

Another process that we would like an experimentalist to look at (because we cannot possibly calculate it accurately) is

$$O^+ + O \longrightarrow O + O^+. \tag{21}$$
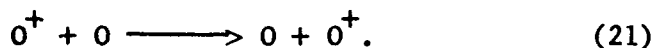
The energy at which this reaction is examined is unimportant; the point is that ambipolar diffusion of an ion moving in its parent gas is essentially controlled by the magnitude of the charge-transfer cross section. If the charge-transfer cross section at some energy is available, then the theoretical description is quite reliable for extrapolating this down to thermal energy. Then we will be able to compute the ambipolar diffusion coefficient - a very important parameter in dealing with the motion and behavior of the upper regions. In discussing the atmosphere, one also has to consider spatial motion; so we would like to know the ambipolar diffusion coefficients. All I would like to know is this: Would somebody please try to measure (21) so that I can calibrate my calculations?

I would like to make one other point on the explanation of the green line in the airglow. In the past, we have had the famous Chapman mechanism of three-body recombination,

$$O + O + O \longrightarrow O_2 + O(^1S), \qquad (22)$$
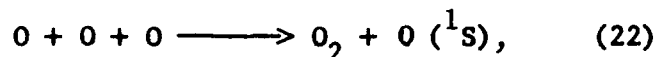
giving rise to the green line. A very strong argument in favor of this reaction is that the green line is observed throughout the night. Therefore, the excitation mechanism must involve a major constituent. There is an enormous reservoir of atomic oxygen produced during the day which recombines slowly at night; hence (22) should be ample. The one thing that is necessary, of course, is that the rate should not be too small. This did not seem to present any difficulty; if anything, the problem was that perhaps it was too large. It has been measured now by two different people who get extraordinarily _small_ rates. What they do is in some way to try to observe this reaction for a certain density of atomic oxygen. They don't observe any green line at all. This result gives them a maximum possible rate. Using that maximum possible rate, they find that this reaction is not sufficient to explain the airglow intensity, I think they do have some suggestion for explaining it, but I feel that by and large one must be very suspicious of negative results. They are saying that they cannot observe the reaction; therefore, the rate is very small. One might argue that they cannot observe it because in some way they are destroying it. So one always likes to have a positive result. I know the measurement has been made by two different people in two different places quite independently by two different methods, but I would like very much to have a third person try it as well.

3. SOME PROBLEMS CONNECTED WITH THE ANALYSIS

OF THE STRUCTURE OF THE SOLAR ATMOSPHERE

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I should like to summarize some problems connected with the analysis of the structure of the solar atmosphere from the standpoint of the physicist interested in the atomic parameters most needed by the astronomer. Let me first review briefly the nature of the observed solar spectrum. Then I will summarize the classical method of analysis of the atmosphere. Finally, I will remark on what some of us have called the "new spectroscopy," and the greater demand it places on the atomic parameters needed.

3.1. Observed Solar Spectrum

We distinguish two regions of the spectrum: that lying in the region $\lambda\lambda$ 2985 - 30,000A, the visible spectrum, which we study from observatories on the ground; and that outside this region, which we study from rocket and satellite observations made above most of the earth's atmosphere.

a. Visible Spectrum

The visible spectrum falls into two parts, according to the method of study: the disk spectrum - the Fraunhofer spectrum - studied simply by training a telescope on the solar disk; and the spectrum of the outer solar atmosphere, studied mainly at eclipses and by means of a coronagraph. The disk spectrum is composed of the continuum and of superposed absorption lines and bands. The lines arise mainly from neutral and singly ionized atoms such as Fe, Ti, V, Cr, etc.; and the bands arise mainly from diatomic molecules such as CN, CH, OH, etc. These lines and bands are produced chiefly in the photosphere, slightly higher than in the regions of production of the continuum, but not differing too greatly in physical conditions. The temperature is about 6000° where the continuum arises; about 5000° where these lines are formed. The electronic density is of the order 10^{13} - 10^{14}. There remain still some problems of identification of some of the fainter absorption features, which are probably molecular in origin. The central cores of some of the stronger lines probably originate in the lower parts of the outer solar atmosphere, in the chromosphere.

The spectrum of the outer solar atmosphere is, when studied at the limb at time of eclipse, an emission spectrum. It is mainly a line spectrum, with a faint continuum at lower heights. You find in emission almost all the lines that you find in absorption in the Fraunhofer spectrum, but a greater relative number of lines from singly ionized metals. Also, you find for the first time in this region - called the chromosphere - lines of He I and He II. The structure of the chromosphere is still not known completely. The uncertainty is not too great up to some 1500 km above the limb; the electron temperature, T_e, increases from a minimum of some $4000 - 4500^\circ$ at the limb to about $10,000^\circ$ at the 1500 km elevation. The value of electron density, n_e, drops to about 10^{11} at this height; while the hydrogen density, n_H, drops from about 10^{16} to about 10^{11}. Above this region, there is evidence of the onset of strong inhomogeneities in atmospheric structure; possibly T_e reaches $30,000 - 50,000^\circ$ in regions just above the 1500 km level.

In the upper parts of the outer atmosphere - the corona, beginning somewhere between 5,000 and 10,000 km lines were observed from highly ionized atoms such as Fe X - Fe XIV, Ca XV, etc. These lines are forbidden lines, arising mainly in the ground configuration of the ion. The ionization level ranges from some 235 to some 815 ev; the excitation level of the observed lines is some 3 - 6 ev. A number of faint coronal lines of this type remain to be identified. To produce these ions, one requires T_e of the order 10^6. There is presently some discrepancy about the exact value; line profiles suggest a kinetic temperature of about 2×10^6; ionization calculations suggest $T_e \lesssim 1 \times 10^6$. The values of n_e in the region of line formation are also somewhat uncertain, of the order $10^{10} - 10^8$. So, in the visible spectrum of the outer atmosphere, we find nothing between He II, with an ionization potential of 54 ev, and Fe X, with an ionization potential of 265 ev. To fill the gap between them, we need to go to the far-ultraviolet spectral regions, where observation requires getting beyond the earth's atmosphere.

b. Rocket Ultraviolet Spectrum

Observations from rockets have extended our knowledge of the solar spectrum from λ 2985A down to some 84A; measures by photon counters have extended the range further, to some few angstroms. This spectral region can again be divided into two parts: $\lambda\lambda$ 2985 - 1800 and $\lambda\lambda$ 1800 - 84.

Between $\lambda\lambda$ 2985 - 1800, we again find an absorption spectrum, analogous to the Fraunhofer spectrum in the visual range. We find many lines, and very many blends; but the identification problem is no greater than in the visible spectrum. Around λ 1800, however, the spectrum changes completely; it becomes entirely an emission spectrum, in the lines. We observe in emission the resonance lines of H, C I, O I, N I, He I, and He II - but also lines of very highly ionized atoms. For example, we find lines of all the ions lying in the Li isoelectronic sequence, from C IV to Si XII. We find all the ions from O I through O VI. To me it is most surprising to find all stages of ionization of silicon between Si I and Si XII (ionization potential, 525 ev). Thus, this ultraviolet spectrum offers the possibility of filling the gap between He II and Fe X mentioned in the study of the visible spectrum, and to see for the first time the allowed lines of coronal ions and forbidden lines of the intercombination type.

We recognize immediately that this emission spectrum cannot arise in the photosphere, as does the majority of the spectrum observed on the disk in the visual region. Thus, these lines refer to different regions of the chromosphere, where the temperature changes very quickly over small height ranges, and its study may permit us to map out this temperature distribution. We also note that the emission spectrum in the rocket region differs in one other sense from that observed in the visual region at the solar limb. At the limb, the atmosphere is not optically thick enough to produce much continuum; so a line in emission there does not have the same significance as does a line in emission in the rocket ultraviolet observations observed on the disk, where the atmosphere is semi-infinite in extent.

There remains now a big job of identification. While the spectrum of highly ionized light atoms has been partially observed in the laboratory with insufficient accuracy, the spectra of coronal ions such as Fe XI, Fe XIII, and Fe XIV have never been observed in the laboratory at all. We have no knowledge, other than theoretical, of the location of the energy levels. We need to know the positions of these energy levels with considerably more accuracy than theoretical methods presently give, and to know them for many elements; e.g., Fe, Ni, Ca, A, Si, Mg, and Al.

In addition to the need for laboratory wavelengths, there are difficulties in identification arising from the lack of knowledge of the absorption by N_2, O_2; and O in the earth's atmosphere. This absorption still seems significant at the 225 km level, the upper limit for the rockets now in regular use.

3.2. Interpretation of the Solar Spectrum

We observe spectral lines - their central intensities, their total intensities, their profiles. From these, we want to deduce the distribution of electron temperature and electron density, the chemical abundance of the elements, and information on velocity fields in the atmosphere. Let me begin with the classical approach Seaton has already outlined, then turn to the "new spectroscopy".

a. Classical Analytical Approach

This is the simplest possible basis for analysis - one introduces the assumption of Local Thermodynamic Equilibrium. Everywhere the atmosphere is described by a set of thermodynamic equilibrium parameters - temperature, density, pressure - and all microscopic distribution functions are those of thermodynamic equilibrium with values given locally by the local values of the thermodynamic equilibrium parameters. Thus, we can use the Boltzmann, Saha, Guldberg-Waage, and Maxwell laws. All we need to know is a model of the atmosphere - the distribution of T and n_e - and chemical abundances. Then everything can be computed and compared with observations. The intensity of an absorption line depends on - in addition to the atmospheric model - the number of atoms in the lower state, the f-value, and the broadening mechanism.

The classical analysis is usually based upon an interpretation of the total intensity of the line. This approach arose in the days when only inaccurate line profiles existed, and from the need for a rather gross analysis. It is usually called the "curve-of-growth analysis," and consists in looking at the change in total absorption as the number of absorbing atoms increases. So what one does is to compare two curves - one theoretical, one observational. The theoretical curve is a plot of total theoretical absorption in the line as a function of Ngf, where N is the total number of absorbing atoms along a column in the atmosphere above the region of formation of the continuum, and g is the statistical weight of the absorbing level. The observational curve is a plot of total observed absorption in the line as a function of gf-value. One uses several lines of a multiplet to construct segments of the observational curve, and then adjusts these segments to construct the complete curve. The adjustment gives a measure of "excitation temperature" in the Boltzmann law, and also a measure of the random microscopic velocity broadening of the absorption coefficient. The form of the curve for large gf-values gives a measure of the collisional broadening in the wings by comparison with the theoretical curves.

Thus, you see that we need reliable values for many f-values, both for strong and for weak lines. It is not sufficient to have relative values within a multiplet - we need also absolute values, particularly when we finally want to determine absolute chemical abundances. In the literature, we find wide discrepancies in f-values, not just between theoretical and empirical values, but within each of these categories. This is particularly true for complex ions.

-23-

Besides these f-values, we need to know the details of what I will call collective processes. We need to know details of line-broadening mechanisms due to the presence of other atoms. In almost all cases, the central parts of the lines are broadened by Doppler effects; but in the photosphere, in the metallic lines, collisional broadening plays a large role. Such broadening comes mainly from van der Waals broadening by hydrogen atoms. The constants used are sometimes measured, sometimes computed with a very approximate formula.

We need also to be able to compute the partition functions and the perturbation in them due to the presence or other atoms. Computations by different authors of such effects show variations of a factor 10. So, particularly for higher temperatures, we need much more information on the partition functions.

Another broadening problem is of course the Stark effect, since hydrogen and helium are such important ions astronomically. Even though much work has been done in recent years (notably by Griem and by Kolb), the asymptotic formulae are valid to only about 10 percent accuracy in the far wings, and often this is not enough for good studies of the profile. Also, the approximations are not valid for Balmer lines of high series number, which we do observe, however. So work remains to be done.

Finally, we note that the ultimate test of the adequacy of this classical approach is the consistency of the results. How well do the "excitation" temperatures determined from the analysis agree, at the same point in the atmosphere, from ion to ion, from line to line, and with T_e determined from the continuum? We see that the precision in determination of such T_{ex} values reflects directly the precision in determination of the atomic constants, particularly the f-values. Comparison of T_{ex} with T_e also reflects uncertainty in the atmospheric model, particularly in the higher layers where we cannot derive an empirical model from the continuum, but must rely wholly on the lines. But then, for a check as to whether inconsistency reflects an error in the analysis, or in the atomic constants, or in the assumption of LTE - or even possibly a departure of the solar atmosphere from the usually assumed spherical symmetry - we must carefully check in detail each of the constants, each of the assumptions we have built into the analysis. So I make a plea for more and better atomic constants, and turn to summarize the attempts to proceed other than by the LTE assumption and the problems involved in this approach.

3.3. The New Spectroscopy

The essence of this approach is that we no longer assume LTE. Consequently, we cannot assume that we know the microscopic distribution functions at each point of the atmosphere, but we must introduce some method to compute them. We must admit the possibility that these distribution functions depend strongly upon the transfer of radiation through the atmosphere, and thus upon the particular atmospheric region considered and upon its optical properties. In this case, we expect that the distribution functions may be quite different when we consider atmospheres of differing chemical compositions, or when we consider extended atmospheres and atmospheres of limited extent. We no longer can apply the standard spectroscopic experience gained in the laboratory and the solar photosphere for line identification - the assumption that the lines arise in more or less homogeneous surroundings, with a knowledge of temperature, statistical weight of level, and f-value sufficing to give an idea of relative intensity of lines. We replace this with the recognition that the relative intensity of lines is as much a function of the over-all structure of the atmosphere as it is of the energy-level structure of the ion; and as much a function of the other atomic rate parameters as it is of the f-values only. This we call the "new spectroscopy".

Because most of the spectra of stellar atmospheres remain constant in time (the relaxation times of astronomical phenomena are generally very large compared with the relaxation times of atomic processes), we replace the assumption of LTE by the condition of statistically steady-state population of the atomic energy levels. So we must write down all the rate processes and use these to solve the equations $dn_i/dt = 0$ for occupation numbers n_i of the ith energy level. Consequently, in addition to the parameters required in the LTE analysis - which were the atmospheric model, f-values, line-broadening mechanisms - we must know inelastic collision cross sections (mainly electron-atom, only rarely atom-atom) and the radiation field in all lines and continua at each point in the atmosphere. We do carry over the assumption of one LTE distribution function, the Maxwellian law for velocity distribution of electrons and atoms, because the relaxation time for elastic collisions is so short.

Thus we can distinguish two kinds of problem to be solved. The gathering of all atomic rate parameters is a problem for the physicist. The solution of the statistically steady-state equations is a problem for the astronomers. Actually, we can break the solutions of these equations into two broad classes: optically thin and optically thick atmospheric regions. We mentioned that we must know the radiation field in all lines and continua. If the atmospheric region considered is everywhere so transparent that it has a negligible effect on the radiation field, then the radiation field can be treated as a known parameter in computing radiative rates; and the solution of the statistical equilibrium equations is straightforward. If, however, the radiation

field is determined in significant part by the atmospheric region being studied, then one must solve an equation of radiation transfer for each frequency, simultaneously with solving the equations of statistical equilibrium. The two kinds of equation are completely coupled. We have for the transfer equation in a strong spectral line, as Seaton has already written:

$$\mu \frac{dI_\nu}{d\tau_\nu} = I_\nu - S_\nu \qquad (1)$$

$$S_\nu = B_{\nu_0}(T_{ex}) \, j_\nu/\varphi_\nu \qquad (2)$$

S_ν is the source-function, and describes the interaction between radiation field and material. T_{ex} is the excitation temperature, which is expressed in terms of the occupation numbers of the energy levels by

$$B_{\nu_0}(T_{ex}) = (\frac{2h\nu^3}{c^2})\left[\frac{n_L}{n_U}\frac{g_U}{g_L} - 1\right]^{-1} \qquad (3)$$

where L and U stand for lower and upper states of the transition, respectively, and the n_k are occupation numbers of the energy levels. The symbols j_ν and φ_ν are normalized profiles of the emission and absorption coefficients, respectively. In the central parts of lines, j_ν/φ_ν can be shown equal to unity; the situation in the wings is presently uncertain and we require detailed consideration of the broadening mechanism. So T_{ex} comes from a solution of the statistical equilibrium equations, and must be used in equation (1) to determine I_ν, which in turn enters the statistical equilibrium equations in determining radiative rates. There are n equations of statistical equilibrium if we consider an ion with n levels plus a continuum; there are n (n+1)/2 radiative transfer equations, including transfer in the continuum. These simultaneous solutions of transfer and statistical equilibrium equations are difficult, and only simple cases have been solved thus far. But these are problems for the astronomers - before we can even try to solve them, we need from the physicists all the atomic rate coefficients.

This non-LTE structure has been applied mainly to the study of the outer solar atmosphere. There the continuum is weak; we deal with emission lines and one studies the solutions of the equations without the complicating presence of the continuum. Moreover, the atmospheric regions, containing a given ion, are very often limited in extent. However, we need an enormously greater range of atomic constants than is required for the LTE studies of the photosphere. As already mentioned, we still need the f-values. Now, however, we need them not only for the neutral and singly ionized metals, but for all the ions mentioned in discussing the chromospheric, coronal, and rocket ultra-violet spectra. In addition, we need now all the inelastic collisional cross sections. There seems little controversy among astronomers that these regions require the non-LTE treatment, the "new spectroscopy". What remains, however, is to get experience in treating these problems; only the simplest have yet been touched.

Although the analysis of the disk spectrum has proceeded largely from the classical standpoint, there are some who question the validity of its application, even under the higher densities of the photosphere. Here the continuum is strong, and one studies the formation of absorption lines. Essentially no theoretical work has been done on the non-LTE methods in the photosphere; what investigations exist are almost wholly empirical. The atmosphere is semi-infinite in opacity, although the regions where the absorbing atom are concentrated may, in certain cases, be limited.

3.4. Conclusions

Let me try to select for you what I think are the outstanding needs for the solar astrophysicist in the way of atomic parameters. We can use, of course, any new information on location of energy levels, f-values, collision cross sections, and what I have called the mechanisms of collective processes. But let me try to emphasize those needs that I think are particularly outstanding.

I would say that the outstanding problem in the analysis of disk spectra of the sun and stars, where the observations refer mainly to the photospheric regions, is a test of the validity of the LTE assumption. If it is wrong, we may expect very sizable errors in both abundance determination (some estimates run to errors of a factor 4) and in atmospheric model in regions where we cannot use the continuum. For this test, we need accurate absolute f-values for ions such as Ti I, Ti II, V I, V II, Cr I, and Cr II. These ions are of different spectroscopic configurations and behave in different ways in the photosphere, so they should provide sensitive tests. If we can show that the presence of non-LTE effects already found empirically by some workers do not come only from bad f-values, we will have changed very considerably our ideas on how to analyze the great bulk of spectral data studied. There are also questions of possible inhomogeneity and of various kinds of velocity fields that are involved with these analyses.

A second problem lies in the kind of non-LTE effects to be expected from the ionized metals in the chromosphere. Such a study will give much information on the structure of the low chromosphere, $h \lesssim 2000$ km, where the possibility of inhomogeneous structure first arises. We should like to know f-values and collisional cross sections for such ions as Ti II, Sr II, and Mn II.

I should remark that the attempts by Seaton and van Regemorter seem to promise "cooking" formulae for collision cross sections for these ions that are reliable within a factor 2 or so. These seem very, very useful. I want to emphasize that our interpretation is very sensitive to these cross sections: an error by a factor 2 in cross section gives a 50 percent error in predicted intensity. So we would be deeply grateful for any improvement, even in isolated cases, over this uncertainty of a factor 2.

A third problem is that of the discrepancy in T_e and/or T_k measured in the corona. We have wondered a long time whether it was the ionization collision cross sections that were wrong, which would result in an error in T_e. But it now seems that these cross sections cannot be wrong to that extent. So we ask whether the interpretation of the profile is wrong, or whether we have incorrectly computed the coupling between excitation and ionization processes in the corona. One suggestion is that the corona is heated by shock waves; could we have two temperatures in the region of the shocks? Or, are there other inhomogeneities in the corona which would give the effect?

A fourth problem is somewhat, though not exactly, similar. When we try to use Fe X, Fe XI, Fe XIII, Fe XIV, and Fe XV to determine temperatures, we find almost as many values of T_e as we find lines. So, since not only ionization but also excitation collision cross sections are involved, could we be seriously in error in the latter, particularly for forbidden transitions? We have been accustomed to using a very old formula, presented by Menzel and Hebb, for forbidden transitions in the ground configuration. An accurate calculation has been made in only one case - that of Fe XIV, by Blaha, and a change of a factor 11 over the Menzel-Hebb result was found. So we ask, is this factor 11 good for all ions, or does it, as seems probable, depend strongly on the configuration? We also have again the problem that perhaps these lines from different ions come from different regions - the possibility of a strongly inhomogeneous corona.

A fifth problem concerns what I will call a new theory of excitation. Because our observations were for a long time confined to the visual part of the spectrum, we have become accustomed to working only in the ground configuration of these ions. That is, we consider only the levels 3-6 ev out of the ground state, while the ionization is some 300 ev. But we find in the ultraviolet region a great many lines from higher excited levels. Our first computations show these lines have appreciable influence on exciting the lower levels, which we have heretofore assumed isolated. It turns out that not only do we have to consider collisional excitation of these higher levels, with cascade to the lower levels, but also the effect of built-up emission in the resonance lines from these higher levels and hence radiative excitation. This kind of treatment is a very important part of the "new spectroscopy," and we have just become aware of it. So, while existing treatments of the excitation of coronal lines from the non-LTE viewpoint have adopted the "thin-atmosphere" approach (radiation field known a priori), this approach no longer seems valid. Again, we have need for the positions of these energy levels and then for f-values and collision cross sections.

I want to stress very strongly this problem of a proper method of computing the excitation, and then the interpretation of these coronal lines. Mainly for Fe X and Fe XIV, but also now for some other ions, there is a great deal of routine daily observation. We obtain many data in this way, and it is not clear at the moment that we know how to use them. Published values of electron temperature and kinetic temperature, and emphasis on the discrepancy between them, can be very misleading if we are not certain of the basis for our interpretation. Also, these data represent a very great deal of effort at a number of observatories, and we are eager to get the most from this activity. So I emphasize to you, as an example, that since we do not even have good terms for the excited levels of Fe XIV out of the ground configuration, we can hardly make the necessary investigations to be sure of the basis upon which the interpretation of these routine observations rests.

I put in what I call the first urgency all those problems that will clarify the physics of the solar atmosphere. After that, I would put what I would call numerical problems. These latter will change particular results, but they will not change very deeply the methods by which we analyze and try to understand the solar atmosphere.

Although it is not a problem in atomic constants, except insofar as they aid interpretation, I want to stress that we should like to get much more information on the ultraviolet spectrum, because it covers such a wide range in energy and therefore physical conditions. For example, we have already mentioned that He II carries us through the low chromosphere to regions where T_e is maybe some $50,000^\circ K$. And we noted the presence of strong lines in the Li I isoelectronic sequence including C IV, N V, and O VI. A rough ionization equilibrium of C IV will, as Seaton told us, give $T_e \sim 70,000^\circ K$. A rough computation shows N V gives $\sim 120,000^\circ K$; from O VI, we estimate $\sim 200,000^\circ K$. Thus we find a continuous progression of ions, covering the range from the temperatures found in eclipse studies in the low chromosphere, and progressing toward those found in studies of the coronal ions. We really hope to get information from this ultraviolet spectrum on the intermediate layers of the solar atmosphere that have hitherto been inaccessible to us.

Finally, I note that preliminary interpretation of the central core of the Ly α line of H suggests its formation in a region where $T_e \sim 70,000$ - $100,000^\circ K$, so we overlap the information possible from the above-mentioned lines. We have a good theory for the formation of H Ly α, which rests strongly on the inelastic collision cross section, and this quantity seems reasonably well known for Ly α. But I would also note that precisely the same kind of analysis can be made of the H and K lines of Ca II and the Mg II lines near λ 2800. All these lines from these three ions are what we call in the new spectroscopy "collision-dominated," and give good information on the distribution of T_e. So again, we would like very much to have the necessary atomic constants for Ca II and Mg II. In this way, we can slowly map the structure of the solar atmosphere.

I have asked data from everybody - physicists working on atomic constants, upper atmosphere people working on solar ultraviolet observations. I have tried only to show you a rough picture, and why the data are needed. We are involved in many analytical problems, and the accuracy of the solutions depends upon the accuracy of atomic constants. If we are off by a factor 10 in physical data, detailed analytical mathematical results based on these constants are worthless.

U. S. DEPARTMENT OF COMMERCE
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NATIONAL BUREAU OF STANDARDS
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Radio Standards. High Frequency Electrical Standards. Radio Broadcast Service. Radio and Microwave Materials. Atomic Frequency and Time Interval Standards. Electronic Calibration Center. Millimeter-Wave Research. Microwave Circuit Standards.

Radio Systems. Applied Electromagnetic Theory. High Frequency and Very High Frequency Research. Modulation Research. Antenna Research. Navigation Systems.

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